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EXAMINER	
TSANG FOSTER, SUSY N	

ART UNIT	PAPER NUMBER
1745	

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Please find below and/or attached an Office communication concerning this application or proceeding.

Advisory Action Before the Filing of an Appeal Brief	**Application No.** 09/669,877	**Applicant(s)** MILLS, RANDELL L.
	Examiner Susy N. Tsang-Foster	**Art Unit** 1745

--The MAILING DATE of this communication appears on the cover sheet with the correspondence address --

THE REPLY FILED 22 October 2004 FAILS TO PLACE THIS APPLICATION IN CONDITION FOR ALLOWANCE.

1. ☒ The reply was filed after a final rejection, but prior to or on the same day as filing a Notice of Appeal. To avoid abandonment of this application, applicant must timely file one of the following replies: (1) an amendment, affidavit, or other evidence, which places the application in condition for allowance; (2) a Notice of Appeal (with appeal fee) in compliance with 37 CFR 41.31; or (3) a Request for Continued Examination (RCE) in compliance with 37 CFR 1.114. The reply must be filed within one of the following time periods:
   a) ☒ The period for reply expires 3 months from the mailing date of the final rejection.
   b) ☐ The period for reply expires on: (1) the mailing date of this Advisory Action, or (2) the date set forth in the final rejection, whichever is later. In no event, however, will the statutory period for reply expire later than SIX MONTHS from the mailing date of the final rejection.

   Examiner Note: If box 1 is checked, check either box (a) or (b). ONLY CHECK BOX (b) WHEN THE FIRST REPLY WAS FILED WITHIN TWO MONTHS OF THE FINAL REJECTION. See MPEP 706.07(f).

Extensions of time may be obtained under 37 CFR 1.136(a). The date on which the petition under 37 CFR 1.136(a) and the appropriate extension fee have been filed is the date for purposes of determining the period of extension and the corresponding amount of the fee. The appropriate extension fee under 37 CFR 1.17(a) is calculated from: (1) the expiration date of the shortened statutory period for reply originally set in the final Office action; or (2) as set forth in (b) above, if checked. Any reply received by the Office later than three months after the mailing date of the final rejection, even if timely filed, may reduce any earned patent term adjustment. See 37 CFR 1.704(b).

NOTICE OF APPEAL

2. ☒ The Notice of Appeal was filed on *22 October 2004*. A brief in compliance with 37 CFR 41.37 must be filed within two months of the date of filing the Notice of Appeal (37 CFR 41.37(a)), or any extension thereof (37 CFR 41.37(e)), to avoid dismissal of the appeal. Since a Notice of Appeal has been filed, any reply must be filed within the time period set forth in 37 CFR 41.37(a).

AMENDMENTS

3. ☐ The proposed amendment(s) filed after a final rejection, but prior to the date of filing a brief, will not be entered because
   (a) ☐ They raise new issues that would require further consideration and/or search (see NOTE below);
   (b) ☐ They raise the issue of new matter (see NOTE below);
   (c) ☐ They are not deemed to place the application in better form for appeal by materially reducing or simplifying the issues for appeal; and/or
   (d) ☐ They present additional claims without canceling a corresponding number of finally rejected claims.

   NOTE: _____. (See 37 CFR 1.116 and 41.33(a)).
4. ☐ The amendments are not in compliance with 37 CFR 1.121. See attached Notice of Non-Compliant Amendment (PTOL-324).
5. ☐ Applicant's reply has overcome the following rejection(s): _____.
6. ☐ Newly proposed or amended claim(s) _____ would be allowable if submitted in a separate, timely filed amendment canceling the non-allowable claim(s).
7. ☐ For purposes of appeal, the proposed amendment(s): a) ☐ will not be entered, or b) ☐ will be entered and an explanation of how the new or amended claims would be rejected is provided below or appended.

   The status of the claim(s) is (or will be) as follows:
   Claim(s) allowed: _____.
   Claim(s) objected to: _____.
   Claim(s) rejected: _____.
   Claim(s) withdrawn from consideration: _____.

AFFIDAVIT OR OTHER EVIDENCE

8. ☐ The affidavit or other evidence filed after a final action, but before or on the date of filing a Notice of Appeal will not be entered because applicant failed to provide a showing of good and sufficient reasons why the affidavit or other evidence is necessary and was not earlier presented. See 37 CFR 1.116(e).
9. ☐ The affidavit or other evidence filed after the date of filing a Notice of Appeal, but prior to the date of filing a brief, will not be entered because the affidavit or other evidence failed to overcome all rejections under appeal and/or appellant fails to provide a showing a good and sufficient reasons why it is necessary and was not earlier presented. See 37 CFR 41.33(d)(1).
10. ☒ The affidavit or other evidence is entered. An explanation of the status of the claims after entry is below or attached.

REQUEST FOR RECONSIDERATION/OTHER

11. ☒ The request for reconsideration has been considered but does NOT place the application in condition for allowance because: See Continuation Sheet.
12. ☒ Note the attached Information Disclosure Statement(s). (PTO/SB/08 or PTO-1449) Paper No(s). 20041022
13. ☒ Other: See Continuation Sheet.

Continuation of 13. Other:
Attachments:
1) Appendix in 09/009,837 -Examiner Bernard Souw's Answer to R. Mills' Response to Souw's Appendix, and
2) Notice of References Cited (PTO- 892).

Continuation of 11. does NOT place the application in condition for allowance because:

Claims 1-28 remain rejected for reasons given in the previous Office Action.

Applicant's arguments filed on 10/22/2004 have been fully considered but they are not persuasive.
As stated in the previous office actions, applicant's claimed invention is based on the existence of the hydrino atom which is contrary to the known laws and theories of chemistry and physics. Applicant's theory of the hydrino atom predicts a new form of the hydrogen atom having energy states represented by fractional quantum numbers that are below the conventional ground state of the hydrogen atom. These energy states having fractional quantum numbers are contrary to the conventionally accepted energy states of the hydrogen atom having positive integer quantum numbers predicted by quantum mechanics that have been successfully verified by decades of independent, reproducible experimental results as stated in the ATTACHMENT TO RESPONSE TO APPLICANT'S ARGUMENTS in paper #12 mailed on 3/25/2003 (hereinafter referred to as "ATTACHMENT in paper #12").
With respect to applicant's 72 page response to Souw's Appendix attached to the previous office action, the arguments presented therein are substantially the same as those given in copending case 09/009,837. Therefore, a copy of the Appendix written by Examiner Bernard Souw responding to applicant's arguments in two parts for copending case 09/009,837 is attached herein. Part I responds to the experimental evidence presented by the applicant and Part II responds to the Theoretical Part of applicant's arguments.

With respect to applicant's general 188 page response, applicant mostly repeats his previous arguments which have been addressed by the Examiner as seen in all of the previous responses to applicant's arguments. Therefore, all of the Examiner's previous responses to applicant's arguments of record, and the appendixes included in all the previous office actions to support the Examiner's arguments are incorporated by reference in their entirety into this present response to applicant's arguments.
All of the Examiner's previous office actions and the present attached appendix by Examiner Bernard Souw explain over and over again why applicant's theory is mathematically and physically flawed. Applicant's flawed theory cannot predict the existence of the hydrino and conventional quantum mechanics forbids the theoretical existence of the hydrino. It is also illogical for the applicant to analyze his own experimental data using his flawed hydrino theory to prove the existence of the hydrino atom as stated in the previous office actions. Since applicant's theory is scientifically and mathematically flawed, there is no theoretical foundation for the hydrino atom and all of applicant's data cannot prove what is not theoretically possible. All of applicant's own experimental evidence of record detract from the central issue that the hydrino does not theoretically exist.

Applicant in his present response continues to misinterpret his own evidence of record and cited prior art. The applicant also continues to misinterpret the Examiner's statements. The discussion below and the attached appendix illustrate some of applicant's misinterpretations.

With respect to applicant's response regarding his NMR data, the applicant modifies his own NMR evidence of record by submitting a new declaration by Dr. Turner (filed 10/22/2004). In the previous office action, the Examiner rebutted applicant's conclusion that the upfield shifts in his NMR data are due to the alleged novel hydrino compound by responding that contaminants such as β-MgNiH have the same upfield shifts. In a previous office action of record, the Examiner pointed out that Dr. Turner's original declaration filed on 5/18/2000 states that he has never observed shifts in the region of –4 to –5 ppm in his 20 years of practicing NMR spectroscopy since 1978 except in applicant's samples (a copy of the declaration was attached to the previous office action). Just because Turner himself never observed shifts in the region from –4 to –5 ppm does not provide positive evidence that these are due to novel compounds and not due to any previously known compounds. The Examiner rebutted this statement in Turner's original declaration by citing references to Hayashi (Hayashi, S. et al. (1997) "Accurate determination of 1H Knight shifts in Mg2NiHx and MgHx by means of high-speed magic angle spinning," Journal of Alloys and Compounds, vol. 248, pp. 66-69 (Paper A); Hayashi, S. et al. (1997) "1H NMR and magnetization measurements of a nanostructured composite material of the Mg2Ni-H system synthesized by reactive mechanical grinding," Journal of Alloys and Compounds, vol. 256, pp. 159-165 (Paper B); Hayashi, S. et al. (1997), "Local structures and hydrogen dynamics in amorphous and nanostructured Mg-Ni-H systems as studied by 1H and 2H nuclear magnetic resonance," Journal of Alloys and Compounds, vol. 261, pp. 145-149 (Paper C)) which show that β-MgNiH has transitions in the –4 to – 5 ppm region (see p. 48 of "ATTACHMENT in paper #12").

In response to the Examiner's evidence of record that β-MgNiH have transitions in the –4 to –5 ppm region, Turner now qualifies his original statement in the new declaration by adding a new paragraph that the shifts observed in the region from –4 to –5 ppm are only known to be due to transition metal hydrides such as β-MgNiH but that Ni and Mg were not detected in applicant's sample. Turner does not provide any additional evidence besides relying on the Examiner's provided evidence of β-MgNiH to support his general statement that shifts in the region from –4 to -5 ppm are only known to be due to transition metal hydrides. Turner's current statement now reflects the evidence provided by the Examiner that β-MgNiH have transitions in the region of –4 to –5 ppm. Turner does not provide any solid evidence to support his general statement that upfield shifts in the –4 to –5 ppm region are known only to be due to transition metal hydrides. It is inaccurate and illogical to extrapolate a piece of prior art provided by the Examiner showing β-MgNiH having shifts in the –4 to –5 ppm to the general statement that upfield shifts in the –4 to –5 pm region are known only to be due to transition metal hydrides.

Furthermore, applicant's and Turner's assertions that there are no contaminants in the sample are not convincing because these samples were not purified after the synthesis process. Applicant's experimental syntheses of KH*Cl, KH*Br, and KH*I were made from the corresponding alkali halide KCl, KBr, and K using potassium metal as the catalyst and each compound was prepared in a stainless steel glass cell comprising a Ni screen hydrogen dissociator, catalyst, and alkali halide or alkaline earth hydride (see Experimental section on pp. 966-967 of applicant's paper, Mills et al., "Identification of compounds containing novel hydride ions by nuclear magnetic resonance spectroscopy", International Journal of Hydrogen Energy 26 (2001) pp. 965-979). Ni was used as a hydrogen dissociator and can easily be present as a contaminant such as a nickel hydride containing compound in the resulting products. Turner states in the new declaration that the only compounds known to have chemical shifts at –4.1 and –4.5 ppm are transition metal hydrides. Therefore, it is

the Examiner's position that the peaks at –4.1 ppm and –4.5 ppm can be due to minute amounts of contaminants such as a transition metal hydride containing compound in applicant's samples.

The Examiner notes that Turner's new declaration modifies the old declaration by changing the pulse angle from 15 to 35 in paragraph 7 and adding the new paragraph:

"For sample 080304BLP1, in the H MAS NMR spectrum two unusual signals were observed, at –4.1 and –4.5 ppm. The only compounds known to have chemical shifts in this region are transition metal hydrides, in particular Mg2NiH4. Elemental analysis (Galbraith Laboratories, Inc., Knowville, TN) showed that Mg and Ni are not detected in this sample, and that K was the main metal present. Earlier NMR data has shown that the hydride of K appears at about 1.0 ppm. Therefore, these results suggest that the signals at –4.1 and –4.5 ppm represent a novel species, and do not correspond to any known metal hydride."

This new paragraph in Turner's declaration does not provide conclusive support that these upfield shifts are due to a novel species because all possible known transition compounds other than those including Ni that could have upfield shifts in this region have not been ruled out by the applicant or Turner. This new paragraph only states that K was the main metal present. It silent about what about other metal elements, especially transition metal elements, are present in this sample provided by the applicant. Furthermore, it is also possible that a previously unstudied, ordinary, non-transition metal hydride compound having upfield shifts is present in the sample.

Finally, applicant and Turner both agree that conventional transition metal hydrides have upfield shifts in the –4 to –5 ppm region. Since these NMR signals are due to the hydrogen atom themselves in these conventional transition metal hydrides as measured by solid state proton NMR,and the position of the signal reflects the surrounding electronic environment of the hydrogen proton, the upfield shifts in these known conventional transition metal hydrides are due to hydrogen protons in a certain electronic environment surrounding the hydrogen protons and are not due to any novel states of the hydrogen atom in the compound. Therefore, upfield shifts of protons in solid state proton NMR are known to be due to the electronic environment of the hydrogen proton that do not involve hydrino form of the hydrogen atom. Hydrinos are not necessary to explain the upfield shifts in solid state H NMR as evidenced by known transition metal hydrides having these upfield shifts in the same region.

With respect to applicant's assertion on pages 58-95 of the amendment that there are 51 independent test results, a close examination of these 51 independent test results are mostly applicant's own work or those of his collaborators as stated and addressed in sections 19-25 of the ATTACHMENT IN PAPER #12 and in Part I of the attached appendix. Applicant's own work and those of his collaborators cannot be considered independent test results.

On pages 105-106 of applicant's 188 page response filed on 10/22/2004, applicant asserts that there is an enormous body of additional theoretical support that applicant has submitted for the new states of hydrogen and that the applicant has provided an enormous body of experiment evidence that lower-energy hydrogen states are produced by the disclosed catalytic reaction. However, this assertion is unrelated to the Examiner's argument that there is no theoretical or experimental support for new forms of one electron atoms having an atomic mass of at least four and having an increased binding energy greater than the binding energy of the corresponding ordinary one electron atom because these new forms of one electron atoms having an atomic mass of at least four are not hydrino atoms. Nevertheless, since applicant uses the same mathematically and scientifically flawed theory of the hydrino atom as theoretical support for one-electron atoms having an atomic mass of at least four and having an increased binding energy greater than the binding energy of the corresponding ordinary one electron atom, the Examiner remains unpersuaded that these novel forms of one electron atoms are theoretically supported or actually exist for the same reasons of record given for the hydrino atom.

On page 111 of the response, applicant asserts that there is no contradiction with respect to the enthalpy of reaction of the catalyst throughout his specification. The Examiner remains unpersuaded because the applicant is now introducing new matter and arbitrary values into his postulated equations (not derived as explained in previous Office Actions) in order to explain his contradictions in his original disclosure.

With respect to astrophysical data as support of his hydrino theory, applicant continues to misinterpret the data of Labov and Bowyer on pages 133-138 of the present response where applicant assigns transitions observed by Labov and Bowyer as being due to the hydrino. The astrophysical data provided by Labov and Bowyer can be explained by conventional science without the need to use applicant's scientifically implausible theory of the hydrino atom. As stated in the previous Office Action, according to the document titled "Hydrocatalysis Technical Assessment, Prepared for Pacificorp, prepared by Technology Insights, dated August 2, 1996", Labov and Bowyer dispute applicant's interpretation of their data. The applicant of the present application is the founder of Hydrocatalysis Power Corporation (HPC) now known as Blacklight Power, Inc. Pages 20-21 of the document states that spectral data taken from the reference S. Labov and S. Bowyer, "Spectral Observations of the Extreme Ultraviolet Background", The Astrophysics Journal, 371, 810 (1991), were evaluated by HPC for indications of hydrino. HPC assigned peaks in the wavelength region of 80 to 650 Å to hydrino transitions. As shown in Table 4-1 on page 21 of the document, the HPC assignments contradict the alternative assignments made by the authors of the paper.

Page 21 of the document also states that Bowyer (an astrophysicist and author of the astrophysics journal paper cited above) disputed the HPC interpretation of the data and that the paper on the HPC interpretation submitted to the Astrophysical Letters and Communications was not accepted for publication. The document also states on page 21 that the low energy hydrogen concept and its implications regarding data interpretation has not received general review or acceptance by the astrophysics community. Thus, applicant's assertions regarding the existence
of hydrino based on observations of radiation spectra from space, i.e., astrophysical data, have not been accepted by the astrophysics community since a more credible scientific alternative (that presented by the authors of the astrophysics paper) exists to explain the spectral data.

Applicant continues to misinterpret quantum mechanics (QM) on pages 119+ of the present response. These misinterpretations are rebutted in Part II of the attached appendix and have been rebutted in all of the arguments made by the Examiner in the previous Office actions and previous attached appendixes to the Office actions.

Applicant's arguments on pages 148-157 with respect to the plasma references cited by the Examiner show that he has misinterpreted the Examiner's statements and that he does not understand the crucial point that the Examiner made in the previous office

action regarding applicant's anomalous line broadening data. The Examiner's main point was that microwave plasma experiments containing hydrogen and one of Ar or He do not cause anomalous line broadening in contrast to applicant's data and applicant does not explain why the microwave experiments of Luque and Luggenhoelscher do not cause anomalous line broadening even though hydrogen and Ar or He (H, Ar, and He are regarded as a catalyst in applicant's experiments and theory) are present in the experiments. According to applicant's arguments and his data, a plasma containing Ar and hydrogen would show anomalous line broadening due to the resonance transfer mechanism of Ar with hydrogen but this anomalous lie broadening effect was absent in the microwave experiments of the prior art cited by the Examiner.

It is crucial to note Luque did not observe Ar catalyzing hydrogen atoms in his microwave discharge experiments (that would be evidenced by anomalous line broadening according to applicant's arguments) in direct contrast to applicant's microwave discharge experiments with Ar and hydrogen and applicant does not deny that Luque did not observe anomalous line broadening in his microwave experiments containing Ar and hydrogen.

Applicant has also seriously misinterpreted the Examiner's plasma arguments by incorrectly comparing the Examiner's cited line broadening of 0.16 nm in the prior art with >100 eV hot H found in applicant's rt-plasmas. The applicant's misinterpretation of the Examiner's remarks on his plasma data, those of the cited prior art, and his own data are detailed on pages 6-12 of the attached appendix (Part I, section B (subsections d.1-d.6, e, and f)).

As explained in Part I of the attached appendix, applicant's assertion of anomalous line broadening in his plasma data due to the resonance transfer (r-t) mechanism is not plausible because there are alternative, conventional explanations for this increased line broadening. The plasma sheath effect offered by the prior art is a more plausible explanation for the increased line broadening than applicant's rt mechanism involving the postulated hydrino (see E. Kovacevic et al., "The Dynamic Response of the Plasma on the Dust Formation in Ar/C2H2 RF Discharges" at http://www.icpig.uni-greifswald.de/proceedings/data/Kovacevic_1 and Cvetanovic et al., J. Appl. Phys. 97, 033302-1, 2005 that are both cited in the attached appendix).

In the Cvetanovic reference cited in the attached appendix, those of ordinary skill in the art in the plasma field do not agree with the rt mechanism proposed by the applicant to explain the anomalous broadening in the hydrogen Balmer alpha line (see abstract and pages 033302-1 to 033302-2 of the reference). Instead, the reference states that the excessive Balmer alpha line broadening is related to the collisions of the fast hydrogen atoms with molecular hydrogen and can be explained by the conventional collision model (CM).
The Cvetanovic reference also states that two independent experiments performed simultaneously in two different laboratories have not been able to reproduce the applicant's excessively broad Balmer line shapes in microwave induced discharge (MID) experiments (see page 033302-2, left hand column). These laboratories did not detect excessive broadening in the MID experiments.
As pointed out on page 33302-2 of the Cvetanovic reference, applicant's own plasma results contradict his own theory since his plasma data containing pure H2 only does not show any anomalous line broadening that is inconsistent with his own theory and argument that two hydrogen atoms (that act as a catalyst) can provide a net enthalpy equal to the potential energy of the hydrogen atom (27.2 eV) which is the necessary resonance energy for a third hydrogen atom. As shown the Figures of applicant's document entitled "Comparison of Excessive Balmer α Line of Glow Discharge and Microwave Hydrogen Plasmas with Certain Catalysts" that was cited in the information disclosure statement filed on 7/2002, there is no anomalous line broadening for microwave plasmas of pure hydrogen alone which contradict applicant's own theory and arguments.
Thus, in view of the serious flaws in applicant's theoretical foundation for his invention, the lack of independent, reproducible experiments to verify the existence of the hydrino atom, and the lack of conventional acceptance of the existence of the hydrino atom which is contrary to the accepted scientific theory of the hydrogen atom, applicant has failed to provide preponderance of evidence to support his claims.

Any inquiry concerning this communication or earlier communications should be directed to examiner Susy Tsang-Foster, Ph.D. whose telephone number is (571) 272-1293. The examiner can normally be reached on Monday through Friday from 9:30 AM to 6:00 PM.

If attempts to reach the examiner by telephone are unsuccessful, the examiner's supervisor, Patrick Ryan can be reached at (571) 272-1292.
The fax phone number for the organization where this application or proceeding is assigned is (703) 872-9306.
Information regarding the status of an application may be obtained from the Patent Application Information Retrieval (PAIR) system. Status information for published applications may be obtained from either Private PAIR or Public PAIR. Status information for unpublished applications is available through Private PAIR only. For more information about the PAIR system, see http://pair-direct.uspto.gov. Should you have questions on access to the Private PAIR system, contact the Electronic Business Center (EBC) at 866-217-9197 (toll-free).

st/ Susy Tsang-Foster

SUSY TSANG-FOSTER
PRIMARY EXAMINER

APPENDIX in 09/009,837

EXAMINER BERNARD SOUW'S ANSWER TO R. MILLS' RESPONSE TO SOUW'S APPENDIX –

I. Experimental Part

(A) General Arguments

Most of the "evidences" recited on pgs.1-37 of the 83 page amendment filed 8/11/2004 in response to Dr. Souw's Appendix attached to the previous office action are repeats from Applicant's former writings. Therefore, the examiner's response will be mostly the same. Applicant's alleged "evidence" falls into three categories:

(a) Those published by Applicant himself, his own company Blacklight Power Inc. (hereinafter BLP) and/or its subsidiaries, including companies paid by BLP to do work on BLP's behalf, all of which report results which are in contradiction to those obtained by independent third parties. In this regard, **all** attempts carried out by independent third parties to reproduce Applicant's claimed results have failed [1, 2]. Thus, Applicant's publications of this category are not considered as supports for the patentability of the present invention, since their results are deemed incredible. Falling under this category are publications nos. 7, 13-15 (sponsored by BLP), 17, 20-43 and 46-47.

(b) Those published in non-peer-reviewed journals, as already identified in previous Office Action(s); and

(c) Those claiming observations unrelated and/or irrelevant to hydrino, such as excessive line broadening, novel peaks (either plasma or solid state spectroscopy), excess heat, enhanced radiation, i.e., phenomena explainable by conventional physics (e.g., impurities that evidently disappeared after surface cleaning [3]), while totally lacking any hard evidence (such as material hardness measurement), as already identified in previous Office Actions. To this category belong publications nos. 1-6, 8-12, 16, 18-19, 44, and 45.

The Examiner's rejection of "evidences" of category (a) to (c) remains the same, and is summarized as follows:

(A.1) Peculiarity or anomaly alone is by far not sufficient as "evidence". There are a great abundance of peculiarities and anomalies in this world, from "irreducibly complex molecular machines" to "crop circles". Many are hoaxes, and some are genuine phenomena waiting to be resolved by true science. However, hydrino is here excluded as a possible cause for the peculiarities and anomalies presented on pgs.1-37, not only because there is no evidence for its existence, but additionally, because the underlying theory, the Grand Unified Theory of Classical Quantum Mechanics, hereinafter GUT, has now been proven totally invalid as a scientific theory (see part II of this Appendix) owing to the incredibly large number of mathematical flaws and violations of known physical laws. There are still many plausible causes instead of the incredible hydrino that may be responsible for the peculiarities and anomalies cited in Applicant papers listed on pgs.1-37, a few of which have been discussed in previous Office Action(s) and will be consequently prosecuted in the following sections. To summarize, Applicant's results are either (a) disproved by independent third party researchers (e.g., Marchese et al. [1] and

EarthTech [2]; see B.3(c) below), or (b) explained by others as being due to causes other than hydrino (e.g., Fan et al. [3] and Luggenhoelscher [see previous Appendix]).

Specifically responding to Applicant's statement on pg.17, it is not the Examiner's duty or responsibility to present any alternative explanation; it is sufficient to show that the observed anomaly cannot be due to "hydrino". It is the Examiner's duty and responsibility to reject any mechanism that is scientifically impossible, such as the hypothetical effects due to "hydrino", since there is no evidence that "hydrino" exists, and furthermore, its existence has been proven scientifically impossible. Such a rejection is made possible by the MPEP under 35 U.S.C. § 101 and § 112/¶.1.

(A.2) Applicant's "evidence" is unpersuasive, because NONE of them is hard evidence, but all are invariably argued over some anomalies, such as excessive line broadening, anomalous peaks (in either plasma or solid state spectroscopy), excess heat, enhanced radiation, etc., which do not count, and hence, unpersuasive. Regarding evidence, a claim of strong bonding must be validated by measurement of material hardness, but not through unpersuasive arguments over peculiar lines that are irrelevant for being hardly above the noise level, as done by Applicant. NONE of the experiments done by other independent third party researchers has been able to reproduce Applicant's claimed results [1, 2] (see B.3.b) below).

(A.3) **All** of the alleged evidences are only argued based on the fractional energy level of hydrogen, for which there is no theoretical justification (see Part II of this Appendix: Theory).

(B) Specific Arguments

(B.1) Pg.29

Regarding Applicant's misidentification of the well-known He-II 304 Å line routinely found in solar spectrum as being due to Applicant's non-existent "hydrino" [4] (cited in previous Appendix), the Sun is known to also contain hydrogen and helium. Applicant's attempt to justify Applicant's obvious misidentification of the line by referring to new elements, such as iron, which has no relevance to the disputed 304 Å line, is unpersuasive. In this regard, Applicant's change of argument to "*the observed 304 Å line is not entirely due to ionized helium*" is also unpersuasive because: (1) There is no other element known in the art that may have contributed to the 304 Å line; and (2) It does not remove the fact that Applicant has misidentified the 304 Å line as being due to "hydrino".

(B.2) Pg.30

Again, the Examiner is not required to provide alternative explanation; it is sufficient to prove that Applicant's explanation is incredible (see A.1 above). Since the invention unambiguously claims the effect as being solely due to hydrino, and this hydrino is evidently non-existent, a rejection under 35 U.S.C. § 101 combined with § 112/¶.1 is proper.

(B.3) Pg.33-35

(a) Strong bonding must be evidenced by measurement of material hardness [5], not by mere arguments of anomalies observed in XPS spectral lines. Anomalies may have many other

causes, but not by hydrino. The latter must be excluded, for having neither experimental nor theoretical justification.

(b) Applicant's XPS line anomaly has been identified by an independent third party as an impurity line: it disappeared after surface cleaning [3]. This refutation has been recited in the previous Appendix, but failed to be addressed in Applicant's response. Therefore, Applicant's insistence of this line of being a "hydrino" line remains unpersuasive on both experimental and theoretical grounds.

(c) Pg.36-37

The experiment of Marchese et al. cited by Applicant has proven by hard evidence that the reaction suggested by Applicant is not more efficient than conventional reaction (A. Marchese's **Final Report [1], pg.33, lines 1-2 below Fig.29**).

In addition, EarthTech, which is an independent research company, failed to confirm Applicant's claimed result. EarthTech's effort to replicate Applicant's claim is documented at <http://www.earthtech.org/experiments/blp/prelim.html> [2a], and the negative finding at <http://www.earthtech.org/experiments/mills/mills1.html> [2b]. Based on these two negative results alone among others [2a, 2b], Applicant's arguments on pg.36-37 must be deemed unpersuasive. Consequently, Applicant's claim of having invented a novel, more efficient chemical process, is deemed incredible.

For reasons stated above, publications from A.J. Marchese relating to "hydrino" are not counted as support, but instead, as a refutation of Applicant's claim, in support of the Examiner's. These include "evidence" nos.16 and 44.

(d) Regarding pg. 137-138 of Applicant's main 161 page Response dated 08/11/2004, that the 0.16 nm line broadening cited by the Examiner Souw is allegedly "*negligible to the >10 eV hot H found in Applicant's rt-plasmas*", and further, on pg.142 of 161, "*absolutely negligible compared to the >100 eV hot H found in rt-plasmas*", must be dismissed for the following reasons:

(d.1) The 0.16 nm broadening (equivalent to 3,7 cm^{-1}) is cited by Examiner Souw to be compared with the 0.27 nm broadening measured by Applicant, but not to "*10 eV or 100 eV hot H*" as alleged by Applicant. This purpose is unambiguously clear in this reproduced passage from the Examiner's Appendix attached to the previous action:

*"Secondly, and most importantly, anomalous hydrogen line broadening is not at all an evidence for the existence of hydrino, because it is well known in the art that such a broadening may be caused by many other conventional mechanisms, such as microwave plasma effects, the latter having not been considered by Applicant. Instead, such an effect has been so far ignored or dismissed by Applicant without any valid reason. The measured excessive line-width shown in Applicant's Fig.6 of ref.[6], i.e., **0.27 nm**, is about the same magnitude as what is measured by other authors, e.g., ref.[5] cited in the May 7 Appendix, here reproduced in Fig.1 below.*

*As shown in Fig.[1], the anomalous line width of **0.16 nm**, measured in a microwave discharge similar to Applicant's under the same gas mixture and pressure range, is about 10 times the Doppler width, and has been attributed to microwave plasma effects." (ref. [5] Luggenhoelscher et al.; Ref. [6] Mills et al.)*

Obviously, Applicant has misrepresented the original dispute over Applicant's 0.27 linewidth by changing or shifting the original subject matter into something else (translational kinetic energy; see below).

(d.2) By reciting 10 eV on pg.138, but 100 eV on pg.142, not only has Applicant compared to a differently related quantity (presumed translational kinetic energy; see next), but also has Applicant failed to particularly point out the subject matter he wants to raise (10 eV or 100 eV?).

(d.3) Applicant is silent about writing the 3.7 cm^{-1} linewidth in wavelength unit. The alternative expression, $\delta\lambda \approx 0.16$ nm, obviates Applicant's $\delta\lambda \approx 0.27$ nm, without ever postulating or presuming any Doppler effect. Instead, Applicant chose to express the observed line width in [eV] unit, which is simply obtained by multiplying the linewidth originally in units of wavenumber (3.7 cm^{-1}) with $c=3\cdot10^{10}$ cm/sec, thus resulting in $\delta\nu \approx 100$ GHz, and further multiplying with the Planck constant $h=4\cdot10^{-15}$ eV·sec to give approximately $h\cdot\delta\nu \approx 0.45$ meV. While the expression $h\nu$ bears the physical meaning of a kinetic energy of an oscillating electron having a frequency ν, the new quantity $h\cdot\delta\nu$ would mean a blur or spread in the oscillation kinetic energy of a radiating electron transition dipole, the latter being a QM entity without classical correspondence ($=\langle\Psi_2|\ \mathbf{a}\cdot\mathbf{D}\ |\Psi_1\rangle$; see original Appendix, sect.3/pg.7). This blur may be due to Stark effect or microwave effect or something else that does not need to be further specified at this point. However, Applicant proceeds to improperly compare this line width with a hypothetical 10-100 eV translational kinetic energy, which is not just in a different unit, but of a totally different nature involving a sequence of presumptions that is not only controversial, but also disputable, as will be described next. Thus, Applicant is comparing "apples" to "oranges".

(d.4) Applicant (mis)interpret the observed linewidth as a Doppler width, for which there is no justification, but --at most-- only a presumption or tentative suggestion. To recapitulate, Applicant came to the 10-100 eV number by firstly presuming the observed linewidth as being entirely due to Doppler effect. Secondly, Applicant then converts the frequency shift (100 GHz) corresponding to the observed line broadening into atomic velocity, then finally multiplying the square of this velocity by the atomic mass to derive the suggested 10-100 eV translational kinetic energy (which is totally of different nature than the 0.45 meV blur or spread of unknown origin in the oscillation kinetic energy of a radiating electron transition dipole, as recited above). Such a derivation is based on a sequence of presumptions that may be partially or even entirely incorrect. Although Doppler effect is omnipresent, there is no justification for assuming the observed line broadening as being entirely due to the Doppler effect. The factual evidence only shows a 0.16 nm line width as observed by Luggenhoelscher, comparable to a 0.27 nm claimed by Applicant. There is no evidence that Applicant's 0.27 nm can be correlated to a translational kinetic energy of ">10 eV" or ">100 eV", or whatsoever, by presuming the linewidth were entirely caused by "***hot H***", as postulated by Applicant. Thus, a correlation of the observed line broadening anomaly with hydrogen translational kinetic energy, or velocity, or Doppler effect, is NOT a FACT, but only a suggestion or preposition, as correctly stated by Kovacevic et al. [6] by using the wording "probably" and "possible process", seeing that there are still other mechanisms also probable. As a matter of fact, the plasma sheath effect proposed by Kovacevic et al. in [6] sounds even much more plausible than Applicant's postulated hydrino. While it is not the job of the PTO to participate in a scientific debate, a plausibility consideration is here appropriate. Kovacevic's plasma sheath is more plausible, simply because plasma sheath is a

well known fact [7] routinely observed by many other researchers in a large number of unrelated phenomena, as opposed to "hydrino", whose existence is unproven by any evidence, and even more, in violation of known laws of physics, while also being postulated under an incredibly large number of mathematical flaws and conceptual misunderstanding.

Thus, while 0.16 nm and 0.27 nm are scientific facts, Applicant's "10 eV or 100 eV ***hot H***" is ***not*** a scientific ***fact***, since the relation to translational kinetic velocity or energy (Doppler effect) of the radiating atom is only presumed without valid evidence (see Kovacevic [6]). Valid as hard evidence would be, e.g., a Doppler-free laser spectroscopic measurement, such as what was done by the Examiner in previously cited Ref.[8]. This would indisputably separate the Doppler effect from the homogeneous line broadening, the latter including Stark effects and microwave effects. Without such a hard evidence, Applicant's claim of "*10 eV or 100 eV hot H*" remains a hypothesis. Furthermore, such a claim does not have any relevance to, let alone a justification for, the existence of "hydrino". It is thus concluded, Applicant's claim that the observed anomalous hydrogen line broadening were due to "hydrino" remains scientifically *incredible*, justifying the previously applied §101 and §112/¶.1 claim rejections.

(d.5) Applicant's method of estimating the 10-100 eV kinetic energy will now be applied to the Examiner's 0.16 nm linewidth (measured as full width at half maximum, FWHM), showing the sequence of presumptions thereby made, without regards of the validity of Applicant's unverified Doppler presumption. Firstly, the linewidth 3.7 cm^{-1} or 0.16 nm is converted into atomic velocity <v> according to the well known Doppler-shift formula $\delta\lambda/\lambda=v/c$, presuming firstly there is no other contributing effects, and secondly, that the homogenous linewidth is negligible. By taking account for a factor originating from the relationship between a presumed

Maxwell-Boltzman velocity distribution and the definition of FWHM Doppler linewidth, one easily obtains a 1-dimensional average hydrogen translational linear velocity $\langle v_z \rangle$. Presuming further that the velocity distribution is isotropic and 3-dimensional, this translational linear velocity corresponds to an average (3-dimensional) translational kinetic energy of $KE=m\langle v^2 \rangle/2$, where m is the mass of atomic hydrogen ($=1.67 \cdot 10^{-24}$ gm). Ready-to-use formulas that may be taken for the above estimates are, for example, $\delta\lambda/\lambda=\delta\nu/\nu=(1/c)\cdot\sqrt{(8kT\cdot\ln 2/m)}$ [9] and $KE=m\langle v^2 \rangle/2=3kT/2$ [10], in terms of the temperature T as a redundant parameter. One of ordinary skill in the art easily obtains a translational kinetic energy of KE=***15.2 eV***, which properly corresponds to the 0.16 nm line width under the presumptions described above.

We see, this **15.2 eV** kinetic energy is very much comparable to Applicant's **10-100 eV**, just in the same manner as 0.16 nm is comparable to 0.27 nm. Thus, by writing a directly measured linewidth 3.7 cm^{-1} in an alternative unit, 0.45 meV (which itself does not make sense), and then comparing the latter with a hypothesized 10 eV translational kinetic energy, not only is Applicant making an improper comparison, but Applicant is also violating a conceptual fundament of physics, like comparing "apples" with "oranges".

(d.6) Applicant's lengthy discussion on various broadening mechanism conducted on pgs. 139-142 is well known in the art, and is not argued by the Examiner. Disputed is here the interpretation of the observed broadening as being due to atomic velocity, or translational kinetic energy, or Doppler effect. The latter is no more than a probable mechanism, as correctly stated by Kovacevic [6] by using the wording "probably" and "possible process". There are many

other possibilities that would also explain the observed effect, e.g., the well known microwave effect proposed by other researchers, e.g., Luggenhoelscher, as cited previously. Applicant is totally silent about this microwave effects.

(e) Applicant's statement on pg.139, lines 1-3, that "*Stark broadening of hydrogen lines can not be measured at low electron densities* ...", is scientifically inaccurate. Stark broadening, or any homogeneous line broadening, such as due to microwave effects, can well be accurately measured (to 10^{-5} nm or even better), e.g., by means of Doppler-free Laser Spectroscopy, as demonstrated by the Examiner in his own work cited previously [8]. Such a measurement would have been scientifically acceptable as hard evidence for the Doppler effect (but not for "hydrino"), since the Doppler-free technique would be able to cancel out the Doppler effect, thereby measuring only the intrinsic/homogeneous broadening (e.g., natural broadening, Stark broadening, both static and dynamic, AC Stark effect, microwave effects, etc.).

(f) Applicant's argument on pg.140-142 regarding Luque's and Luggenhoelscher's references has no merit, not only because the references are not cited by the Examiner to refute Applicant's incorrect claim of the Doppler effect and "hydrino" (this is accomplished by Kovacevic's [6] by virtue of the plasma sheath effect), but instead, to compare with the 0.27 nm line broadening measured by Applicant (see previous recitation from Applicant's paper). However, irrespective of the validity of Applicant's unverified Doppler assumption, a proper conversion of Luggenhoelscher's line broadening leads to a comparable magnitude (15.2 eV) with Applicant's claimed 10 eV kinetic energy, as previously demonstrated by the Examiner.

Any further argument over line broadening in applicant's data of record will be considered unpersuasive for the reasons given in section I.B.3.d(5).

(C) CONCLUSION

Not a single independent third party (one that is not funded by or in collaboration with applicant) has been able to confirm Applicant's claim(s). Therefore, serious doubts are raised as to the scientific reproducibility of Applicant's results. This situation is very similar to cold fusion, the latter having ultimately ended up with a final dismissal by the scientific community. Since Applicant's invention violates what is conventionally accepted in science, it is not patentable. Such an "invention" is also not useful, since it cannot be reproduced and used by others. Therefore, a rejection under § 101 and § 112/¶.1 is here proper.

In summary, Applicant's claims on hydrino-based processes have neither a credible experimental confirmation nor a scientific basis (see also Part II of this Appendix: Theoretical).

(D) REFERENCES *(those already cited in previous Appendix or by Applicant are printed in italics)*

[1] A.J. Marchese et al., "The Blacklight Rocket Engine" Phase I Final Report, available at <http://engineering.rowan.edu/~marchese/>, also at <http://www.niac.usra.edu>.

[2a] EarthTech Reports, <http://www.earthtech.org/experiments/blp/prelim.html>.

[2b] EarthTech Reports, <http://www.earthtech.org/experiments/mills/mills1.html>.

[3] Y. Fan et al., "X-ray Photoelectron Spectroscopy Studies of CVD Diamond Films", Surf. Interface Anal. 2002, 34, pp 703-707 (see previous Appendix)

[4] He-II in Solar spectrum (see previous Appendix)

[5] "Material Hardness", http://www.calce.umd.edu/general/Facilities/Hardness_ad_.htm

[6] E. Kovacevic et al., "The Dynamic Response of the Plasma on the Dust Formation in Ar/C_2H_2 RF Discharges" at http://www.icpig.uni-greifswald.de/proceedings/data/Kovacevic_1.

[7] Cvetanovic et al., *J. Appl. Phys.* 97, 033302-1, 2005.

[8] E.-K. Souw et al., "The Zeeman Splitting of the 5876 Å Helium Line Studied By Means of a Tunable Dye Laser", Physica 113C, 203, 1982

[9] http://omm.hut.fi/optics/l_o/2004/luennot/spectroscopy.pdf

[10] http://hyperphysics.phy-astr.gsu.edu/hbase/kinetic/molke.html

EXAMINER BERNARD SOUW'S ANSWER TO R. MILLS' RESPONSE TO SOUW'S APPENDIX –

II. Theoretical Part

Applicant's response does not remove any of the Examiner's refutations of his Grand Unified Theory of Quantum Mechanics, hereinafter GUT, as presented in the original Souw Appendix included in the previous office action. Rather, Applicant's response adds a large number of new mathematical and physical errors. Because those new errors are numerous, it is not possible to analyze them one by one without ending up writing hundreds of pages. Therefore, as done with GUT in the previous Appendix, only the significant ones will be addressed in the following sections, which are divided into the same paragraphs or sections as in the previous Appendix.

1. Regarding the derivation of hydrino's fractional energy levels, E_n

(a) Applicant's arguments regarding GUT, Ch. 1-2, 5-6, as recited in his response on pg.37 are unpersuasive: Applicant's formula for E_n is not derived, but postulated, just as stated by the Examiner so far. First-principle means, the principal formula must come out of mathematical derivation. Thus, applicant's formula is not from first principles. It is to be known, that postulate is acceptable in science (e.g., QM), insofar it is supported by experimental evidence and does not contradict with known natural laws. This is not the case with the hydrino. Its existence is not supported by experimental evidence and is also in violation of quantum mechanics (QM), electrodynamics, and the relativity theory.

(b) As already demonstrated in said Appendix, those GUT chapters are full of mathematical flaws and violations of elementary principles of physics, some of which have been previously discussed and will be consequently prosecuted in the following sections.

2. Regarding the alleged "electrostatic Schrödinger Equation (SE)" and "stationary electron"

(a) Applicant has misrepresented the Examiner's previous statements as none of the wording alleged by Applicant, i.e., "electrostatic Schrödinger Equation (SE)" and "stationary electron" is recited by the Examiner in said Appendix. As such, the Examiner is not giving any weight to these arguments thus presented.

(b) Applicant has misunderstood the fundamental QM concept of "stationary state" (see original Souw Appendix pg.1/ sect.2/lines 1 and 3), in which the term "stationary" (or "static") simply means "does not change with time" (as defined in the Appendix pg.2/line 1). This "stationary state" is a fundamental concept that can be found in every QM textbook (see, e.g., McQuarrie [1], Ch.4.3, pg.121, lines 2-3 from bottom, "*Thus, the probability density and the averages calculated from Eq.4-19 are independent of time, and the $\psi_n(x)$ are called stationary-state wave functions*". As such, the wording "stationary state" would never be misinterpreted as "motionless electron" by any one of ordinary skill in the art. As known in the art, an electron in a stationary state is in motion, wherein the motion, or velocity, is inherent in the wavefunction, and is represented by the eigenvalue (for a single state) or expectation value (for a superposition of states) of the particle momentum operator ***p*** (operators are written in ***bold italics***) divided by

the mass m (scalar non-operator), i.e., $\boldsymbol{V}=\boldsymbol{p}/m$, such that the particle velocity is $\langle \boldsymbol{V} \rangle = (1/m)\cdot\langle \psi^*, \boldsymbol{p}\psi \rangle$, in which the operator $\boldsymbol{p}$ is represented by $-i\hbar$ **grad** in the Schrödinger representation. The "stationary" state or "static" probability density (to be distinguished from Applicant's stationary electron) is a direct consequence of the uncertainty relation (see original Souw Appendix/lines 8-10), since the energy E of the state, and also its angular momentum L, are sharply defined, i.e., $\delta E=0$ and $\delta L=0$, which consequently leads to $\delta t=\infty$ (does not change with time) and $\delta\varphi=2\pi$ (total uncertainty of angular position φ), the latter because the angular momentum operator is defined as $\boldsymbol{L}=i\hbar\partial/\partial\boldsymbol{\varphi}$ (or by scalar operator $\boldsymbol{L}^2$), and the complementary of the angular momentum is the angular position φ, which is equivalent to the complementarity between $\boldsymbol{p}$ and $\boldsymbol{x}$, leading to $\delta x=\infty$ for $\delta p=0$ in case of linear momentum and position. In simple terms understandable to those ordinarily skilled in the art, an electron plane wave represented by $\psi \sim \exp(ikx-i\omega t)$ also results in a stationary probability (charge) density, $\rho = |\psi|^2 =$ constant in time, i.e., static, per definition. However, the electron itself (to be distinguished from its state or probability density) is not stationary or static, but instead, moving with a momentum of $p=\hbar k$ and a kinetic energy of $E=\hbar^2k^2/2m$. This is a most basic element of QM well known to-those ordinarily skilled in the art. For these reasons, Applicant's "refutation" of the QM are unpersuasive.

The QM method of calculating spectral line intensities based on vector- and tensor-operators as presented, e.g., by Condon E.U. & Shortley G.H., "The Theory of Atomic Spectra", Cambridge 1967, pp. 45-69, and 112-147 [2], has been mathematically implemented and experimentally verified by the Examiner himself in his two previously cited works [3, 4]. The experimental verification involving hundreds of spectral lines as functions of electric/magnetic

fields was made without a single error or failure. The results were extremely accurate within less than 10^{-5} nm, which is far more superior to the 0.1 nm accuracy achieved in Applicant's measurements. As a proof for the correctness of conventional QM, similar mathematical verifications have been also demonstrated by a great number of other authors. In this regard, a reference to the Examiner's own work is here to be considered important, so as to exclude the possibility of an invalid dismissal from Applicant's side, such as "the Examiner misunderstands his own reference". As already brought up in the previous Appendix, Applicant's Grand Unified Theory (GUT) wave function is incapable of calculating line splitting and line intensities, including line absorption cross-sections, as the conventional QM is evidently capable of (see [2], [3] and [4]). Applicant is invited to present detailed step-by-step calculations showing how his theory is capable of predicting the line intensities and applicant has not done so to date.

(c) Regarding pg.40 of the amendment, the Examiner's argument has been (and is), that not only the ground state, but all stationary states must be also non-radiative in consequence of the Haus theorem, since their probability density distribution does not change with time (i.e., per definition, stationary; see previous Appendix section 2, lines 1-2). To "see" an electron physically moving around an atom, a wave packet has to be constructed as a superposition of stationary states having not only a plurality of orbital quantum numbers (L,m), as described in the original Souw Appendix, sect.2, but also involving at least two principal quantum numbers, n_1 and n_2, as discussed in the original Appendix sect.3. Only then, can a non-vanishing time dependence of the probability density be established, i.e., by virtue of the cross-term $\rho = |\psi|^2 \sim \exp i(\omega_1-\omega_2)\cdot t$ (Note: the energy of a free hydrogen atom, and hence, its frequency, $\omega_n=E_n/\hbar$, only depends on the principal quantum number n). This corresponds to the transition probability

discussed in sect.3 of the original Appendix, which also agrees with the Haus's condition, that a free hydrogen atom composed of at least two eigenstates of different principal quantum numbers does radiate, i.e., making a transition from n_2-state to n_1-state.

This conclusion regarding stationary states is a direct consequence of the Heisenberg Uncertainty Principle, and has been made by the Examiner independent from --but in agreement with-- Feynman and other authorities in QM, the latter contended by the Applicant himself (see 2.d.(5) below). In contrast, Applicant's theory based on point electron, as recited in GUT and on pg.39 is incorrect, since it is in total contradiction to and not reconcilable with the routine experimental observations of electron wave properties, such as interference effects that have found many useful applications, e.g., Reflection High Energy Electron Diffraction (RHEED) and Low Energy Electron Diffraction (LEED).

(d) On pg.39, applicant presents new arguments that the Examiner takes issue with as follows:

(1) Applicant's analysis based on Haus theorem is mathematically and physically flawed, as already addressed in the previous Souw Appendix, to be again repeated and emphasized in the following sections (i.e., mathematically, regarding Applicant's "solution" of electron wave function $\rho(r,t)$ based on the δ-function that does not satisfy the wave equation; and physically, the non-applicability of Lorentz contraction formula to Applicant's orbiting electron).

(2) Applicant's allegation that QM is inconsistent with experimental observation is doubly flawed. Firstly, the fact that hydrogen ground state (n=1) does not radiate is confirmed by experimental observations without a single exception, as already recited in the previous

Appendix. Secondly, Applicant's insistence that the n=1 state *does radiate* is not supported by any valid experimental evidence. Applicant's own "experimental evidence" (if any) must be disqualified, because it can not be confirmed by any independent third party researcher.

(3) Applicant's arguments based on Laloë's article [5] are unpersuasive for reasons to be discussed in a section 6, sub-paragraph (d) below.

(4) Reference [80] is to be disqualified, since it is written by Applicant based on his own flawed theory which has been addressed numerous times by the Examiner.

(5) The proof given by Feynman that has removed the problem of self-radiation in an orbiting electron by virtue of the Heisenberg Uncertainty Principle (HUP) is scientifically convincing and well-accepted by the scientific community, while having been also independently confirmed based on exactly the same reason by the Examiner in the previous Appendix (same section 2, pg.2, lines 1-10; see also sect. 2.c above). This means, the scientific community generally agrees with Feynman and the Examiner, but disagrees with Applicant.

3. Regarding the alleged instability of the (excited) states

Applicant does not adequately address the Examiner's refutation as recited in the previous Appendix, but keeps repeating and insisting the correctness of his Grand Unified Theory (GUT). Applicant misunderstands the QM by sticking to the viewpoint of classical physics, instead of properly reconciling both viewpoints under the correspondence principle. Applicant's misinterpretation of "stationary states" in QM has been adequately described previously. As recited in the previous Appendix, Applicant's formulas (1.59) to (1.68), as well

as Eq. (1) to (5) on pg.44-45, are mathematically flawed and physically incorrect, not only with regard to QM, but also with respect to (Maxwell's) electrodynamics and Einstein's relativity theory, as already described in the previous Appendix and in Sect.10 below. Similarly, Applicant's arguments regarding the instability of excited states based on Quantum Electrodynamics (QED) and Dirac's theory must be disqualified, since Applicant has evidently misunderstood the most basic element of the Dirac theory, specifically regarding the physical concept and the mathematics of Dirac's 4-vector, as described in more details in section 6, sub-paragraph (c). Therefore, Applicant's argument on this subject matter remains unpersuasive.

4. Regarding "Applicant misunderstands the most basic fundamentals of the QM theory"

(a) Applicant's attempt to argue that Applicant's electron wave function $\rho(r,t)$ involving δ-function does not need to satisfy --or must not be a solution of-- the wave equation (pg.45) is totally unacceptable, and hence, unpersuasive because applicant's response contradicts the mathematical requirement that any valid solution must satisfy the generic equation.

Applicant's insistence that his δ-function-based "solution" $\rho(r,t)$ does not need to satisfy --or must not be a solution of-- the wave equation, violates the basic laws of physics and mathematics. It must be emphasized that the entire physics and mathematics that have been developed since Newton and Leibniz form together a non-self-contradictory entity generally accepted by the scientific community. It is a high barrier to disprove what is accepted by conventional science, such as QM (Quantum Mechanics).

Since Applicant's GUT is entirely based on this δ-function-based electron wave function ρ(r,t) which is not a solution of his own starting wave equation, Applicant's flawed GUT does not provide any theoretical support to this patent application. Any further attempt to argue the patentability of his application by relying on GUT will be dismissed as UNPERSUASIVE with referral to this section, ***II.4.a***.

(b) Applicant's angular momentum wave functions (instead of eigenfunctions), as derived in GUT and partly reproduced on pg.58-64, are mathematically flawed and in direct violation of the conventional QM, as already described in the previous Appendix. It turns out, Applicant's rejection of QM is solely caused by Applicant's misunderstanding and misinterpretation of the QM, the latter having been acknowledged in the art as being the most successful theory in the whole history of physics. The validity of QM has been quantitatively verified by multiple generations of physicists/scientists and by thousands, if not millions of phenomena and effects encountered in science and technology. In contrast, applicant's flawed "theory" has not been verified even by a single experiment conducted by an independent third party to date. Thus, Applicant's argument regarding alleged flaws in QM is unpersuasive.

(c) Applicant's remark, "*there is no a priori basis for any theory to be correct*", does not contradict the Examiner's view. However, there are plenty of a priori basis for a theory to be incorrect, e.g., if the theory is incredible, illogical and/or self-contradictory, such as Applicant's GUT and hydrino theory. The Examiner's view on Applicant's theory and experimental evidence is totally different than Applicant's: (a) A correct scientific theory must be mathematically and conceptually self-consistent, and should not contain self-contradiction, e.g., mathematical flaws. In this regard, Applicant's entire theory, as documented in the GUT

document, contains an unprecedented amount of mathematical flaws and errors, as already demonstrated in the previous Appendixes included in all the office actions of record, some of which are now repeated, confirmed and emphasized. (b) A correct scientific concept must be proven by experimental evidence. In this regard, NONE of Applicant's "experimental evidence" is scientifically valid, as already discussed by the Primary Examiner(s) in his/her main Office Action. Applicant's alleged "evidence" falls into three categories, which have been discussed in Part I and already presented in the previous Appendix.

5. Regarding Applicant's misunderstanding of Haus's non-radiative condition

(a) On pg. 51/lines 4-5, Applicant recites: "*a time dependent charge corresponds to a current*". This is just one of the unprecedented number of mathematical flaws and misunderstanding of elementary physical concepts in Applicant's GUT. The mathematical flaw lies in the fact that a current $\boldsymbol{J}$ is a vector quantity (or field), whereas $\rho(\boldsymbol{r},t)$ is a scalar, so they can never be the same as claimed by Applicant ($\boldsymbol{J} \neq \partial\rho/\partial t$, since the left hand side is a vector and the right hand side is a scalar). The physical flaw lies in the fact that they are fundamentally of different natures. Only together (hence their different natures!) they form the charge conservation law, i.e., by virtue of the well known formula $\boldsymbol{div} \cdot \boldsymbol{J} + \partial\rho/\partial t = 0$ (note the scalar operation $\boldsymbol{div}\cdot$ on vector $\boldsymbol{J}$; not $\boldsymbol{J}$ itself). The GUT is completely silence on such mathematical relation and/or operation. Hence, any hindsight argument in this direction from Applicant's side inevitably would be automatically considered invalid and unpersuasive.

(b) In GUT, as well as on pg.51/ff of 83, Applicant's Eqs.1-39 through 1.45 are mathematically flawed, as already recited in the previous Appendix, sect.4/pg.3/lines 8-12 and pg.4/lines 9. One of ordinary skill in the art can easily show that Applicant's charge density $\rho(\boldsymbol{r},t)$ is neither a solution of the Maxwell/Helmholtz equation in terms of Laplace operator nor the Schrödinger equation, i.e., by virtue of the fully analytical integral representation of the δ-function that can be mathematically treated in a rigorous manner (see original Appendix, section 4). Not only is this another example out of an unprecedented number of mathematical flaws and misunderstanding of elementary physical concepts in Applicant's GUT, but most importantly, a solid proof that Applicant's derivation of the hydrino theory is based on the failure to apply rigorous mathematics as proofs as every physics theory should be based upon

(c) Applicant's Eq. 1.41 to 1.45 are based on an incorrect application -- and is a result of his serious misunderstanding-- of the Special Relativity Theory, specifically regarding the inapplicability of the theory to a circulating electron, as already described in previous Appendix. Applicant has failed to address the Examiner's refutation and show a proper understanding of the Relativity Theory in his response to the Examiner's Appendix (see also last section 10).

(d) Applicant's statement on pg.55 that, "*[t]he distinction between an eigenfunction and a wavefunction comprised of eigenfunctions ..... is due entirely to a mathematical postulate of QM*", is mathematically incorrect: Per definition, eigenfunctions are solutions of an eigenvalue equation. Not only the Schrödinger Equation (SE), but also the electromagnetic wave equation of Helmholtz are eigenvalue equations. Consequently, the monochromatic wave function $\exp i(kx-\omega t)$ is an eigenfunction solution of the wave equation, and a wave packet can be constructed

as a superposition of such eigenfunctions. Applicant's GUT theory is based on applicant's serious misunderstanding in this crucial subject matter.

6. **Applicant's confusion regarding wavefunction and eigenfunction**

(a) Due to applicant's misunderstanding of eigenfunctions (see above), applicant then proceeds to separate the physics of angular momentum from its mathematics (e.g., on pg.54-55, and once again on pg.64). A most important characteristic of modern science (ever since Newton) is, that physics must be quantitatively expressed in rigorous mathematics (besides it must be also experimentally verifiable, independent of time, location and observer). The mathematical basis for the QM concept, including the complementary property of position and momentum as well as the Heisenberg uncertainty principle (HUP), is the Fourier Transform, in which both the HUP as well as the concept of eigenfunctions, as distinguished from a superposition (wavepacket), can be intuitively grasped by one of ordinary skill in the art.

(b) On pg.55/lines 8-10 from bottom, Applicant's statement regarding the impossibility of zero rotational energy in case of zero angular momentum (L=0) has no basis whatsoever, and hence, is here dismissed and disregarded. For L=0, the wavefunction is known to be spherical symmetric, meaning that the electron is everywhere within $0 \leq (\theta,\varphi) \leq 2\pi$ with equal probability . To "see" an electron density probability that is inhomogeneous over the angle coordinates (θ,φ), a superposition of angular momenta eigenfunctions is necessary, as described in the original Appendix, which also means that $\delta L>0$ and the system is no longer spherical-symmetric. A spherical-symmetric system (L=0) has a zero angular momentum, since $\boldsymbol{L}^2 Y_{L,m}(\theta,\varphi) = 0$ for L=0, and $\boldsymbol{L} Y_{0,0}(\theta,\varphi) = (\boldsymbol{r} \times \boldsymbol{p}) Y_{0,0}(\theta,\varphi)$ with $\boldsymbol{p}$ being a differential operator (defined by McQuarrie [1]

Eqs.6-81 & 6-83), is also identical to zero, since $Y_{0,0}(\theta,\varphi)$ is a constant (see previous Appendix pg.5-6). Consequently, the rotational energy, $E_R=L(L+1)\hbar^2/2I$ (McQuarrie [1] Eq.6-61/pg.219), is also zero for L=0, whereas $E_R=\hbar^2/I$ for L=1, in direct contradiction to Applicant's claim that the lowest rotational energy is $E_R=\hbar^2/2mr^2$, as recited on pg.55 lines 24-25. Applicant has obviously misunderstood his own cited reference McQuarrie [1], i.e., by inserting L=1 (but not L=0) and $I=mr^2$ in Eq.6-61 on pg. 219 and 209, where r is there NOT the radius of hydrogen atom as Applicant would like to mean, but (r is) the inter-atomic distance in a diatomic molecule, whereas Applicant's m, or McQuarrie's μ, is its reduced mass, as recited in [1] on pg.212/Example 6-5. It is also clear that L=0 is inclusive in the complete set, as recited in Eq.6-60 in [1] on pg.209. McQuarrie [1] discusses in §6-5 to §6-7 the Rigid Rotator model, unambiguously reciting in the title of §6-5 that the Rigid Rotator is a Model for a Rotating Diatomic Molecule ([1]/pgs.210-221). Hydrogen atom is handled by McQuarrie [1] in §6-8 on pg.221 ff.. As stated by McQuarrie [1] on pgs.222-223, Eqs. 6-99 & 6-100, the energy of a hydrogen electron for different quantum numbers (n,L,m) in the absence of magnetic field is degenerate in (L,m), as recited on pg.225, line 20-22 of § 6-9, i.e., it depends only on the principal quantum n, with L satisfying $0\leq L\leq n-1$ (Eq.6-101 in [1]/pg.223), i.e., L=0 also inclusive. Obviously, Applicant's has misunderstood the zero angular momentum case in his own cited reference, McQuarrie [1], for misinterpreting $Y_{0,0}$ as being a spin eigenfunction (GUT, Eqs.1.61-1.65) based on his erroneous understanding that L=0, or zero rotational energy, is impossible, as recited by Applicant on (pg.55, lines 24-25).

The Examiner also takes issue with applicant's removal of $Y_{0,0}$ out of the complete set of angular momentum eigenfunctions $Y_{L,m}(\theta,\varphi)$. As known in the art, the solutions of an

eigenvalue equation form altogether a complete set of eigenfunctions. By taking out $Y_{0,0}$, Applicant's incomplete set of $Y_{L,m}(\theta,\varphi)$ (L,m>0) is now incapable of representing an arbitrary function of (θ,φ), since it is a mathematical rule generally known in the art that an arbitrary function (emphasis on the arbitrary) can only be represented by a complete set of eigenfunctions with all possible values of L, from L=0 to L=∞. Thus L=0 cannot be taken out, as done by Applicant. In view of these serious misunderstandings by the applicant, applicant's arguments on angular momentum and spin are unpersuasive.

Still on pg.55, Applicant's statement "*the Examiner's requirement of taking linear combinations of eigenfunctions to result in a wavefunction solution* ***to avoid violating the Uncertainty Principle*** ", is another example of Applicant's misunderstanding of the Uncertainty Principle, as once again manifested on pg.65 of the amendment discussed below. Either a superposition of eigenfunctions, or a single eigenfunction, are **both** valid manifestations of the Uncertainty Principle, $\delta p \cdot \delta x \approx \hbar$ or $\delta L \cdot \delta \varphi \approx \hbar$, for any two complementary observables. **None** of them violates the Uncertainty Principle, as contended by Applicant. See also the same conceptual mistake in sub-paragraph 6(d) below.

(c) Applicant's angular momentum wave functions as postulated (but not derived) in the GUT and repeated on pg.58-64 are mathematically flawed, since they contain mathematical inconsistencies and self-contradictions, as discussed in previous Souw Appendix (sect.6/pg.5-7). Accordingly, Applicant's argument regarding this subject matter is unpersuasive.

As pointed out in the previous Appendix (sections 6-8 on pgs. 5-9), Applicant is representing both the spin function ($Y_{0,0}$) and the orbital momentum function ($Y_{l,,m}$, hereinafter denoted by

$Y_{L,m}$) in the same space (***r***,t), i.e., as a single function $Y = Y_{0,0} + Y_{L,m}$ (see GUT, Eqs.1.61-1.65). This is a direct contradiction to Applicant's arguments in his present Response, recited on pg.57, (citation:) "*It is physically correct and mathematically correct to solve spin and orbital functions independently, since there is no a priori reason, why they have to be a single eigenfunction or product **[sic!]** of eigenfunctions. After all, they are independent physical phenomena. The two dimensional wave equation plus time is given by McQuarrie* [1]").

Most of this statement has been being practiced in science all the time by those ordinarily skilled in the art, except for one which is denoted with "***[sic!]***". However, Applicant has obviously misinterpreted his own statement, based on Applicant's own cited reference, i.e., McQuarrie [1] for reasons given in the next paragraph.

(c.1) Firstly, McQuarrie's spin-orbital eigenfunction $\Psi_{100\pm½}$, as defined in Eqs.8-50 and 8-51, is a product of the orbital eigenfunction Ψ_{100} (see Table 6-5 on pg. 224) and the spin eigenfunction α and/or β, the latter defined independently by Eqs. 8-43 and 8-46. In contradiction to Applicant's misunderstanding, it is just because it is product, can the resulting wavefunction remain an eigenfunction of both the angular and the spin operators! Thus, that part of Applicant's statement denoted by ***[sic!]*** is fundamentally incorrect.

(c.2) Secondly, Applicant's new statement cited above is a contradiction to Applicant's angular momentum (spin-orbital) wave function given in GUT, Eqs.1.61-1.65, in which the spin wavefunction ($Y_{0,0}$) and the orbital wavefunction ($Y_{L,m}$) are both solutions of the same equation, and represented by one spin-orbital function in the form of an addition of two functions in the same and single (***r***,t) space, i.e., $Y = Y_{0,0} + Y_{L,m}$, but not in two independent functions, $\Psi = \psi \cdot \alpha$ and

$\Psi=\psi\cdot\beta$ as correctly stated by McQuarrie in Eq. 8-50. What Applicant would mean with McQuarrie's "*two dimensional wave equation*" has its solution defined in a two-dimensional space as a (2-dimensional vector) functions α and β defined in McQuarrie's Eq. 8-43. These α and β are known in the art as representing two linearly independent eigenfunctions, or basis vectors, that can (but not must) be conveniently represented by $\boldsymbol{\alpha}$=[1,0] and $\boldsymbol{\beta}$=[0,1], which are obviously orthogonal for satisfying the orthogonality condition in Eq.8-46 on pg.300, and yet fully different than -- and fully independent of -- the ordinary space ($\boldsymbol{r}$,t). *(Note: As generally known in the art, McQuarrie's orthogonality condition in the form of integrals over a not-further-specified spin variable σ (Eq.8-46) is greatly simplified by defining --with Pauli-- the spin functions α and β in its equivalent vector form, $\boldsymbol{\alpha}$ and $\boldsymbol{\beta}$, which is mathematically more elegant and also conventional).* In contrast, although Applicant's $Y_{0,0}$ is constant, it is still a function defined in the same and single space($\boldsymbol{r}$,t) as $Y_{L,m}$, and hence, does not comply with Applicant's own new statement.

(c.3) Thirdly, Applicant has misrepresented his own cited reference [1], the latter unambiguously reciting on pg.300, "*In a sense, $\alpha= Y_{1/2,+1/2}$ and $\beta= Y_{1/2,-1/2}$, but this is strictly formal association, and α and β, and even $\boldsymbol{S}^2$ and $\boldsymbol{S}_z$, for that matter, do not have to be specified any further.*" Thus, it is principally incorrect to interpret $Y_{1/2,\pm1/2}$ as being the same orbital function $Y_{L,m}$, but with L=½ and m=±½. In fact, it is mathematically impossible to do so, simply because the (bounded) solution of the pertinent differential equation requires L to be an integer (see McQuarrie [1], Eq. 6-101). It is further recited on the next line, "*The functions α and β in Eq.8-43 are called spin eigenfunctions*" ...., *which we write formally as* ..." followed by defining its orthonormal properties in Eq.8-46. As known in the art, it is sufficient and correct to

define the spin functions α and β as in Eq. 8-43, together with their orthogonality condition as defined in Eq.8-46. Obviously, what is correctly meant by McQuarrie with $Y_{1/2,\pm 1/2}$ as *formally* representing the spin functions α and β is not $Y_{0,0}$, as insisted by Applicant in his response and in his GUT (Eqs.1.61-1.65). As generally known in the art what McQuarrie meant with α and β are the Pauli spin eigenfunctions, **α**=[1,0] and **β**=[0,1], respectively, which are column vectors that should be rigorously written in columns, i.e., one component above the other (as used by the Examiner in his cited own work [3] as well by a many other authors), instead of sequential rows, i.e., one component after the other.

This will now be mathematically proven by the Examiner in a rigorous manner. As recited in Ref. [3] already cited by the Examiner in the previous Appendix, and also in [6] as a new/independent reference (in order to convince Applicant that this Pauli matrix formulation is truly an elementary concept generally known to those ordinary skilled in the art), the Pauli spin operators are defined as (with ***bold italics*** denoting operators): $\boldsymbol{S}_x = \hbar/2\ \boldsymbol{\sigma}_x$, $\boldsymbol{S}_y = \hbar/2\ \boldsymbol{\sigma}_y$, $\boldsymbol{S}_z = \hbar/2\ \boldsymbol{\sigma}_x$, and $\boldsymbol{S}^2 = \hbar^2/4\ \boldsymbol{\sigma}^2$, with the Pauli spin matrices $\boldsymbol{\sigma}_x$, $\boldsymbol{\sigma}_y$, $\boldsymbol{\sigma}_z$, and $\boldsymbol{S}^2$ conventionally defined as

$$\boldsymbol{\sigma}_x = \begin{matrix} 0 & 1 \\ 1 & 0 \end{matrix} \qquad \boldsymbol{\sigma}_y = \begin{matrix} 0 & -i \\ i & 0 \end{matrix} \qquad \boldsymbol{\sigma}_z = \begin{matrix} 1 & 0 \\ 0 & -1 \end{matrix} \qquad \text{and} \qquad \boldsymbol{\sigma}^2 = \boldsymbol{\sigma}_x^2 + \boldsymbol{\sigma}_y^2 + \boldsymbol{\sigma}_z^2$$

These Pauli spin matrices **σ** (the **bold** print denotes its vector character) are not to be confused with the unspecified spin variable σ used by McQuarrie in Eqs. 8-46. The latter will not be further used, because it has not been (and cannot be, or does not need to be) further specified, and its role has been adequately taken over by the vectorial properties of the Pauli spin

vectors $\boldsymbol{\alpha}$ and $\boldsymbol{\beta}$. Applying these operators to McQuarrie's spin functions $\alpha(\sigma)$ and $\beta(\sigma)$, which are now conveniently and conventionally represented by $\alpha(\sigma) \rightarrow \boldsymbol{\alpha}=[1,0]$ and $\beta(\sigma) \rightarrow \boldsymbol{\beta}=[0,1]$, both defined as column vectors and both are eigenfunctions of both $\boldsymbol{\sigma}^2$ and σ_z, we easily obtain in terms of rigorous undergraduate mathematics:

$$\boldsymbol{S}_z \boldsymbol{\alpha} = \hbar/2\, \boldsymbol{\sigma}_z \boldsymbol{\alpha} = +\hbar/2\, \boldsymbol{\alpha} \;;\; \boldsymbol{S}_z \boldsymbol{\beta} = \hbar/2\, \boldsymbol{\sigma}_z \boldsymbol{\beta} = -\hbar/2\, \boldsymbol{\beta}\;;\ \text{and}$$

$$\boldsymbol{S}^2 \boldsymbol{\alpha} = \hbar^2/4\, \boldsymbol{\sigma}^2 \boldsymbol{\alpha} = \hbar^2/4\,(1+1+1)\, \boldsymbol{\alpha} = 3\,\hbar^2/4\, \boldsymbol{\alpha}\;;\ \text{as well as } \boldsymbol{S}^2 \boldsymbol{\beta} = \hbar^2/4\, \boldsymbol{\sigma}^2 \boldsymbol{\beta} = \hbar^2/4\,(1+1+1)\, \boldsymbol{\beta}$$

The mathematical relations derived above are in complete agreement with the properties of McQuarrie's spin functions as defined in Eq.8-43. It has been thus proven that Applicant has misunderstood and misrepresented his own cited McQuarrie reference [1], as well as the conventional QM that traditionally makes use of Pauli spin matrices [3, 6].

(c.4) Fourthly, on top of his misunderstanding, Applicant also has misrepresented his own McQuarrie reference by presenting $Y_{0,0}$ in place of McQuarrie's $Y_{1/2,\pm 1/2}$ as spin functions, thus leaving an incomplete set of angular momentum eigenfunctions $Y_{L,m}(\theta,\varphi)$ with $L \geq 1$ by excluding $Y_{0,0}$. It is to be emphasized, McQuarrie's ***formal*** $Y_{1/2,\pm 1/2}$ is not (and never can be; therefore McQuarrie's stress on "formal") a solution of the angular momentum eigenvalue equation, as incorrectly assumed by Applicant by misrepresenting it as $Y_{0,0}$. McQuarrie's $Y_{1/2,\pm 1/2}$ is purely formal, and can never be a true or actual angular momentum eigenfunction, $Y_{L,m}$, in which both L and m must be integers, as generally known in the art (see also McQuarrie [1], Eq.6-101 for one-electron atom as well as Eq.6-61 for a diatomic molecule). As generally known in the art, by

formally denoting the spin function with $Y_{1/2,\pm 1/2}$, McQuarrie's set of angular momentum eigenfunctions still includes the zero orbital eigenfunction, $Y_{0,0}$. As such, McQuarrie's set of orbital eigenfunctions remains intact as a complete set of eigenfunctions, as it must always be. Obviously, Applicant's set of orbital eigenfunctions fails to comply with his own reference [1], and furthermore, violates a fundamental law of mathematics.

(c.5) Fifthly, what is correctly meant by McQuarrie with his wavefunction involving α and β is well known in the art as Pauli wavefunctions represented by 2-dimensional eigenvector with components Ψ^+ and Ψ^- [3, 7, 8], each of which being an independent function of (r,t), i.e., $\Psi^+ = \Psi_{1001/2}(r,t)$, and $\Psi^- = \Psi_{100-1/2}(r,t)$, as presented by McQuarrie in Eq.8-51 on pg.301. These two independent and mutually orthogonal eigenfunctions are most conveniently written in the form of column vector components $\Psi^+ = \alpha Y_{L,m}(r,\varphi)R_{n,L}(r)$ and $\Psi^- = \beta Y_{L,m}(r,\varphi)R_{n,L}(r)$, as recited in Eq.1 of the Examiner's own work [3], as well as in Ref.[7] (Eqs.5.42-47), where α=[1,0]=column vector, β=[0,1]=column vector, $Y_{L,m}(r,\varphi)$ is the conventional orbital angular momentum eigenfunction (=spherical harmonics, with L=0 included (see [1] Eq.6-76 on pg.215), and $R_{n,L}(r)$ is the conventional radial function (=associated Laguerre function, in case of hydrogen wave function; see [1] Eq.6-102 on pg.223). The two eigenvector components $\Psi^+ = \alpha Y_{L,m}R_{n,L}$ and $\Psi^- = \beta Y_{L,m}R_{n,L}$, are generally known in the art as Pauli eigenvectors (components) [3, 7]. Mathematically they are equivalent to McQuarrie's Eq.8-51, in which McQuarrie's spin functions $\alpha(\sigma)$ and $\beta(\sigma)$ have been specifically represented by the Pauli spin vectors α and β, both satisfying the orthogonality condition as given by McQuarrie in Eq.4-46, since $\alpha\cdot\beta = 0 = \beta\cdot\alpha$, $\alpha\cdot\alpha = 1 = \beta\cdot\beta$, $\int d\sigma = 1$, and both also satisfying McQuarrie's eigenvalue equations 8-43.

It has been thus shown, that McQuarrie Ref. [1] perfectly agrees with the Examiner's refutation as presented in the previous Appendix as well as in Examiner's Ref.[3], whereas Applicant's GUT wavefunction does not comply with his own cited reference [1], while also violating fundamental laws of mathematics and physics. Note: Ref.[7, 8] are new citations, to show that the Pauli wave functions, Ψ^+ and Ψ^-, are well-known and widely used in the art, as equivalents to McQuarrie's. Thus, applicant's refutation of conventional QM stems from his own misunderstanding of the subject matter, including his own cited reference [1].

This is not an *a priori* standpoint taken by the Examiner, as alleged by Applicant, but has been conclusively drawn from the unprecedented amount of self-contradictory and erroneous arguments of record presented by Applicant that show Applicant's complete misunderstanding of the QM.

The Examiner also continues to disagree with applicant's repeated recitation (and "refutation"!) of Dirac's formulation of particle with spin ½ in the form of a 4-vector (see e.g., [9] & Drell) , which is known in the art as being a natural (i.e., relativistic) extension of the 2-dimensional Pauli vector wave functions to 4-dimensional Dirac vectors that automatically represents anti-particles. Given that applicant has misunderstood Dirac's relativistic formulation, applicant's argument regarding this issue is unpersuasive.

(d) On pg.65, Applicant's argument regarding $\delta\varphi \rightarrow \infty$ vs. $\delta\varphi \rightarrow 2\pi$ only reflects Applicant's misunderstanding regarding multi-valued functions. Furthermore, Applicant's wording "*in order not to violate the HUP*" does not make sense to those of ordinary skill in the art, since a constant probability density in all space having $\delta x = \infty$ does not violate the HUP at all, but is the

manifestation of HUP (both $\delta p=0 \rightarrow \delta x=\infty$ and $\delta x=0 \rightarrow \delta p=\infty$ strictly obey the HUP, $\delta p \cdot \delta x \approx \hbar$). The same conceptual error has been previously discussed in sub-paragraph 6(b). Such a serious misunderstanding of the HUP ultimately disqualifies Applicant's arguments altogether.

7. Applicant's misunderstanding of the Uncertainty Principle in QM

(a) Unlike the uncertainty of position and linear momentum, there is no $\delta\varphi \rightarrow \infty$ in case of sharply defined angular momentum ($\delta L \rightarrow 0$), but only $\delta\varphi \rightarrow 2\pi$, since $\delta\varphi \rightarrow \infty$ inevitably ends up in being confined within 2π due to the multiple values of the angular variable φ. Applicant's confusion in such a simple problem is another evidence for Applicant's misunderstanding of the HUP.

(b) Applicant's has failed to remove, or even properly address the Examiner's points of refutation in the previous Appendix. Consequently, said refutation remains in force, and is here re-instated by incorporation, in addition to new proofs of errors and misunderstanding encompassed in Applicant's response(s), to be detailed as follows.

(c) There is no such thing as "*mathematics versus physics*" as alleged by Applicant; but rather, the two aspects always develop hand-in-hand (see section 5a(a) above). As known in the art, besides experimental evidence, physics is built on rigorous mathematics.

(d) Applicant's argument regarding the Examiner's "bias by QM" is inappropriate because it is the Examiner's job to understand the scientific principles behind an invention by using tools made available to him by conventionally accepted science. QM is one of those tools that has

been conventionally and objectively accepted by the scientific community. The Examiner plays no role in the scientific community's acceptance of QM.

In each and every instance as evidenced by applicant's response throughout the entire prosecution history of this application, the applicant uses the competitor argument whenever his theory is refuted by any individual who provides sound mathematical and physical arguments based on conventionally accepted science such as QM to disprove applicant's mathematically and physically flawed theory. However, it is must be emphasized that QM is not a competing theory but a conventionally accepted theory. Applicant has not provided any solid evidence that QM is flawed. All of applicant's previous arguments regarding the deficiencies of QM and attempts to disprove QM have been refuted by the Examiner in the previous and current arguments of record.

Regarding Applicant's request to have his applications examined by an Examiner who is "skilled in Maxwell equations", the MPEP states that a rejection may rely upon facts within the examiner's own/personal knowledge or other PTO employee(s); see MPEP 2144.03(C), 37 CFR 1.104(c)(3) and 37 CFR 1.104(d)(2). In this regard, the Examiner's skill in the pertinent art, both theoretical and experimental, is documented in his publication [10]. Note, the cited work has been accomplished by the Examiner 17 years ago, such that a "conflict of interest" argument is without merit.

(e) Applicant's reference to Ref.[80] for alleged "failures" of HUP is unpersuasive, since Ref.[80] is written by Applicant himself, and has been deemed incredible for being full of mathematical flaws and incorrect interpretations of physics principles, as previously discussed.

Applicant's misinterpretation of HUP is obviously also the source for his incorrect understanding of a number of references presented on pg.65 of his Response. Beyond his blind citation of the references, Applicant has failed to identify what he meant with "inconsistency" and "paradox".

"Inconsistency" or "paradox" exists in QM only in philosophical terms, depending on the philosophical standpoint of the individual author who made the statement, primarily with regard to what he/she defines as "reality" (cf. Laloë [5]). For example, the current Copenhagen interpretation of QM --more specifically regarding Schrödinger cat paradox, single particle interference, quantum entanglement, quantum teleportation etc.-- is neither a paradox nor inconsistency, when viewed from the philosophical standpoint of Logical Positivism [11-14] (= a modern version of Hume's classical positivism developed by the Vienna Circle --Bohr, Heisenberg, etc--, and is to date tacitly adopted by most physicists and scientists). Under this philosophical viewpoint, "reality" is defined solely as what is perceived by our five senses, as represented by experimental measurements (see, e.g., R. Nakhmanson, [11]). Thus, it would be nonsense to talk about non-measurable parameters, such as suggested in the EPR paradox by some "hidden variables" and summarized in the well-known Bell's inequalities in consequence of the classical interpretation of "reality" as local realism. As of late, the Bell's theorem has been experimentally disproved in favor of the so-called Copenhagen interpretation of QM as a non-local theory [11, 15]. The Copenhagen interpretation of QM is also compatible with Pragmatism [16], which declares any knowledge on "reality", including scientific theories, as being "correct" only insofar as it is beneficial to human experience (i.e., not only capable of explaining, but also able to predict and control), the latter again referring to the five senses, or, in short, experimental measurements. The Copenhagen interpretation of QM is even compatible

with Kant's metaphysics [15, 17] (foundation of modern philosophy, developed in the 18th century after Newton), which is heavily based on human reasoning (=logic, mathematics) and proves that metaphysical "reality" beyond human five-sense perception is not accessible to human knowledge and/or intelligence, as described by his famous argument of "das Ding an sich", or the thing in itself.

In contrast, Applicant's GUT is essentially incompatible with any of those major philosophical views, since the existence of hydrino is not based on experimental evidence (= five-sense perception), and furthermore, the hydrino can not be justified by reason, for obvious violation of logic/mathematics and known laws of nature. However, it is to be emphasized, philosophy is neither a subject matter of physics nor patent examination (non-statutory subject matter). The purpose of the above discussion is just to show that Applicant has misunderstood his own cited references regarding the alleged inconsistencies and paradoxes in QM given on pg.65.

It is to be emphasized, philosophy is totally irrelevant to science & technology, since it has no impact whatsoever on the "reality" itself. It does not matter whether Applicant considers single photon interference a paradox or not; a single photon that is split into different arms of an interferometer will still generate measurable interference effects. Similarly, an experiment designed to test the Bell theorem will invariably show the theorem is wrong (i.e., there is no hidden variable), no matter whether Applicant rejects a non-local QM theory as paradox, or accept QM as it is. This irrelevancy of philosophical interpretation is commonly shared by those skilled in the art, as also expressed, e.g., by Barth [18] on pg.2, col.2, lines 22-25.

For all the reasons stated above, Applicant's contention that the conventional QM is in "serious trouble" because it allegedly entails unsolvable paradoxes and inconsistencies, hence, needs to be rejected and/or drastically revised, is totally unpersuasive.

8. Applicant's confusion regarding electron spin

Applicant has failed to address the Examiner's refutation in the previous Appendix. Applicant's spin wave function as postulated (but not derived) in GUT and repeated on pg.65-69 is mathematically flawed, since it contains mathematical inconsistencies and self-contradictions, as discussed in the previous Appendix (sect.6/pg.5-7), and more specifically in section 6 above. The Stern-Gerlach experiment has been adequately explained by Goudsmit and Uhlenbeck based on electron spin, which theoretically also agrees with the Pauli theory that represents the wavefunctions of a particle with spin 1/2 as 2-dimensional column-vector functions, Ψ^+ and Ψ^-, known in the art as Pauli wave functions [3,7]. These Pauli functions have been previously shown to be in perfect agreement with the spin functions α and β defined by Applicant's own cited reference [1]. These, however, turned out to disagree with Applicant's statement and formulations, as described above in section 6. Therefore, the Stern-Gerlach experiment does not need Applicant's explanation; not only because the underlying theory is incredible, but also because the explanation and prediction provided by the conventional QM is far more superior, far more quantitative and accurate, and --without falling into self-contradiction-- far more comprehensive than what Applicant has to offer. In this regard, Applicant's attempt to defend his derivation of spin-orbital wave function by combining the spin and orbital functions in one

single function of (r,t) has been proven to be based on a misunderstanding over his own reference McQuarrie [1], specifically with regard to Pauli eigenfunctions, as described above and in section 6. A correct interpretation of this Pauli eigenfunctions has been demonstrated by the Examiner by successful application of the conventional QM, as evidenced by elaborate mathematical calculations of intricate line splitting and intensities that have been experimentally verified to be extremely accurate to better than 10^{-5} nm [3]. This accuracy is far more superior to the 0.1 nm accuracy of Applicant's measurements. Accordingly, Applicant's argument regarding this subject matter is totally unpersuasive.

9. Regarding "Applicant's hydrogen wave function is seriously flawed"

Similar to most of his other remarks, here Applicant does not even try to refute the Examiner's arguments as presented in the previous Appendix, but merely re-iterate his position as already presented in his evidently flawed GUT. The incredibly-large amount of mathematical flaws and incorrect understanding of physical principles ultimately disqualifies the GUT as a scientific theory. Every argument based on GUT is therefore unpersuasive.

10. Regarding Applicant's incorrect application of Einstein's Special Relativity

Applicant's repeat of his GUT derivation is unpersuasive, since it does not address the Examiner's point of refutation as brought up the previous Appendix. The Examiner's refutation was/is, that Applicant's application of Einstein's Relativity Theory to an orbiting electron is fundamentally wrong, since such a system is not an inertial system, and hence, the Lorentz

contraction is not applicable. There appears to be a lack of appreciation by the applicant of the crucial difference between inertial systems and non-inertial systems, which is most fundamental to Einstein's Relativity Theory. Therefore, Applicant's entire argument is unpersuasive.

11. Applicant's failure to respond to specific refutations in the original Souw Appendix

Besides Applicant's failure to persuasively argue against the Examiner's refutation of GUT as raised in the original Appendix, Applicant has left these points un-responded:

(a) Applicant's misinterpretation of the radial function in QM that allegedly goes straight through the nucleus, which is raised by the Examiner in sect. 9 of the original Appendix.

(b) The Examiner's invitation for Applicant to use his GUT to calculate line intensities that are verifiable by experimental measurement, as done by the examiner in his two cited own works [3, 4] remains un-responded.

CONCLUSION:

Applicant's response has failed to remove the Examiner's points of refutation as brought in the original Souw Appendix, some of which having been improperly addressed, or even left-out un-addressed. Consequently, all points of the Examiner's refutation remain in force, and are re-instated herein by incorporation, in addition to the above new proofs of Applicant's errors and misunderstanding brought up in his response(s). The Examiner does not evaluate GUT from an exclusive viewpoint of QM, as alleged by Applicant, but takes account of the fact that GUT is

trying to disagree with QM, i.e., by fully considering every point of Applicant's arguments. Thus, the Examiner has evaluated the GUT on its own merit based on its scientific credibility, i.e., its validity with regard to mathematical basis and experimental evidence. It was found, none of the criteria required by the conventional standard for scientific theory and/or patentable invention has been fulfilled.

REFERENCES *(those already cited in previous Appendix or by Applicant are printed in italics)*:

[1] D.A. McQuarrie, "Quantum Chemistry", University Science Books, CA,1983, relevant parts of Chapters 4, 6, and 8.

[2] E.U. Condon and G.H. Shortley, "The Theory of Atomic Spectra", Cambridge, 1967, **pp. 45-69, and 112-147.**

[3] E.-K. Souw et al., "Calculation of the Combined Zeeman and Translational Stark Effect on the Hα Multiplet", Physica 122C, 353, 1983.

[4] E.-K. Souw et al., "The Zeeman Splitting of the 5876 Å Helium Line Studied By Means of a Tunable Dye Laser", Physica 113C, 203, 1982.

[5] F. Laloë, "Do we really understand quantum mechanics?", Am. J. Physics 69 (6), June 2001, 655-701.

[6] Physics 200-04 Course, "Pauli Spin Matrices", <http://axion.physics.ubc.ca/200-04/pauli-spin.pdf>.

[7] H.G. Kuhn, "Atomic Spectra", Longmans, Green & Co., Ltd., London/Harlow 1969 (2nd ed.).

[8] "The Linear Stark Effect", University of Texas Lecture, <http://farside.ph.utexas.edu/teaching/qm/perturbation/node8.html>

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